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power. On making an approximate calculation of the places of *Mercury* and the Sun by the same method as for the future transits of the planet (*Monthly Notices*, May 1877) I found *Mercury* slightly escaped a passage across the Sun's disk on this occasion.

Upton Helions Rectory, Crediton,
April 9, 1878.

A Comparison of the Observations of Contact of Venus with the Sun's limb in the Transit of 1874, *December* 8, *made at the Royal Observatory, Cape of Good Hope, with the corresponding observations made at the stations in the Parliamentary Report, and a discussion of the results.* By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer, Royal Observatory, Cape of Good Hope.

The Cape observations of the times of contact of *Venus* with the Sun's limb at Egress on 1874, December 8, were immediately after the transit forwarded to Greenwich to be worked up with observations of contact made at the other stations. The Cape observations were not, however, included in the Parliamentary Report, and I should not have felt myself at liberty to anticipate the official publication of the results were it not that some values of the solar parallax, supposed to have been derived from their use, have been published in the *Observatory* Magazine for 1878, February. The value of the solar parallax which is given in that magazine, as the result of a comparison between the Cape observations of the phase ζ, or what I call the "real contact," and the corresponding observations at six stations where the contact was retarded at Egress, is $8''\cdot495$. I feel certain that no such value of the solar parallax can be fairly derived from our observations, and I have taken some trouble to test this matter by direct computation and comparison of the results. At the time of the transit there were only two instruments at the Cape fit for the delicate work of observing contacts for a determination of a value of the solar parallax. I observed the contacts with the 7-inch telescope by Merz and employed a power of about 200; Mr. Finlay, my first assistant, used a good 4-inch telescope by Dollond and employed a power of about 180. The contact was also observed by Mr. G. Maclear with a small theodolite of which the object glass had an aperture of 1·65 inches. He employed a power of only 23. With such optical means it was of course impossible for Mr. Maclear to attempt to discriminate between changes of the Sun's limb which are presented near the point of contact, and his observation is not available for comparison with contacts made with larger instruments and with greater optical power. But I thought that an observation made by a careful

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observer with small optical power might be useful in discussions respecting the nature of the contacts observed.

The following are the contacts observed:—

1874, Dec. 8.

Observer, E. J. Stone.

	Cape Mean Times.			
	h	m	s	
(1)	19	6	25	Slight connection, but not firmly established; good.
(2)	19	7	12	Continuous now; a fair breadth.
(3)	19	8	5	Apparent contact.

At the apparent contact a considerable portion of the Sun's limb was cut out, or the ligament was about one-third of the diameter of *Venus*.

Observer, W. H. Finlay.

	h	m	s	
(2)	19	7	10·7	Permanent connection seen.
(3)	19	8	18·3	Apparent contact; considered rather late.

The time, $19^{h}\ 8^{m}\ 18^{s}{\cdot}3$, has been increased 30 seconds for error of count by Mr. Black. Mr. Black noticed at the time of making the entry that he was then counting from the wrong end of the seconds hand. A note of this and of the effect of the error was made at the time, and before any comparison had been made with other observations.

Observer, G. W. H. Maclear.

h	m	s	
19	7	48	First permanent contact.

There can be no doubt about the selection of the contacts which, at the Cape, correspond to the phase ζ. If we take the mean of my time, $19^{h}\ 7^{m}\ 12^{s}$, and Mr. Finlay's, $19^{h}\ 7^{m}\ 10^{s}{\cdot}7$, we have $19^{h}\ 7^{m}\ 11^{s}{\cdot}35$ Cape mean time for the observed phase.

The following table is extracted from the *Observatory*, 1878, February.

Results from Egress—Phase ζ.

Stations.	Acceleration.	Parallax.	No. obs.	Weight.
	s	″		
Sydney (N.S.W.)	388	8·768	5, 11	77
Goulburn (N.S.W.)	388	8·891	2, 11	54
Woodford (N.S.W.)	382	8·801	1, 11	36
Victoria	406	8·900	3, 11	64
Adelaide	339	8·778	3, 11	57
Cape Observatory	187	8·495	2, 11	30
Port Elizabeth (C.G.H.)	165	8·385	1, 11	21

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Stations.	Retardation. s	Parallax. ″	No. Obs.	Weight.
Mokattam and Suez	594	8·774	4, 17	96
Luxor (Egypt)	562	8·780	3, 17	78
Abbasseyah (Cairo)	594	8·780	1, 17	45
Roorkee (India)	544	8·804	1, 17	45
Madras (India)	426	8·918	1, 17	31
Mussoorie (India)	563	8·597	1, 17	45

I have not all the materials which have been used in the determination of the value 8″·495 placed opposite the Cape Observatory; but, as the Cape observations are only compared with 11 observations of Egress retarded, and as the mean value assigned to the last 4^s scarcely differs from the mean value found from the first 7, if the value 8″·495 be correct some such value must be found from a comparison with the 7 contacts observed at Mokattam and Suez, and Thebes which are printed in the Parliamentary Report.

I have therefore computed the tabular distances between the centres for my own station and for the stations Mokattam and Suez, and Thebes. The value of the solar parallax adopted is 8″·90. The following are my results :—

Mokattam and Suez	15′	$41''{\cdot}679 - 2{\cdot}257\,\delta\pi = C$	4 obs.
Thebes	15	$41{\cdot}783 - 2{\cdot}134\,\delta\pi = C$	3 obs.
Cape	15	$41{\cdot}879 + 0{\cdot}616\,\delta\pi = C$	2 obs.

The agreement between these results is so close that really no sensible correction to the assumed value of the solar parallax would appear necessary. However, if we combine the Cape and Suez we find

$$\delta\pi = -\frac{0{\cdot}200}{2{\cdot}873} = -0{\cdot}069, \quad \text{or} \quad \pi = 8''{\cdot}831;$$

and if we combine the Cape and Thebes, we find

$$\delta\pi = -\frac{0{\cdot}096}{2{\cdot}750} = -0{\cdot}035, \quad \text{or} \quad \pi = 8''{\cdot}865;$$

whence, in the mean,

$$\pi = 8''{\cdot}846 \pm 0{\cdot}22\,\epsilon.$$

The value of the solar parallax thus deduced is well within the probable error, if the true value of the solar parallax be as large as 8″·90.

I am at a loss to understand how the value 8″·495 can have been obtained from the Cape observations, unless the time of Mr. Maclear's contact, $19^h\ 17^m\ 48^s$, has been virtually adopted

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as the time of contact, and a second contact agreeing with this has been obtained by taking Mr. Finlay's apparent contact for a real contact, and neglecting to apply the 30 seconds required for error in count, or by erroneously correcting his time for phase ζ by 30 seconds. But in these cases all my observations must have been deliberately rejected, and Mr. Finlay's unduly dealt with. I can hardly believe that such steps can have been taken in an important investigation, but I can see no other explanation which will account for the value of the solar parallax deduced from the Cape observations.

I will now compare all the three kinds of contact which I observed at the Cape with the corresponding observations given in the Parliamentary Report. The adopted value of the mean solar parallax is 8″·95 which is the value of the Parliamentary Report.

First Phase, or δ.

We have five observations available, four from the Parliamentary Report and my observation at the Cape. The corresponding distances between the centres are

		′	″
	Mr. Hunter, Suez	15	40·08
	Mrs. Campbell, Thebes	15	39·31
(1)	Lieut. Hoggan, Rodriguez	15	40·30
	Lieut. Goodridge, Kerguelen	15	40·61
	Mr. Stone, Cape	15	40·38
	Mean	15	40·14

The Cape observation of this early phase therefore agrees within 0″·24 with the mean of the five observations.

Second phase or ζ.

We have for the distances between the centres

		′	″	
	From 4 obs. at Suez	15	41·46	
	„ 4 „ Thebes	15	41·37	
(2)	„ 5 „ Rodriguez	15	41·43	
	„ 2 „ Cape	15	41·79	Stone & Finlay
	„ 3 „ Kerguelen	15	41·28	
	The mean, with weights, is	15	41·44	

The Cape result therefore only differs 0″·35 from the mean. I have taken for Kerguelen the mean of the three observations by Perry, Sidgreaves, and Goodridge.

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Third phase or η.

We have the following observations of apparent contact :

		′	″	
	2 obs. at Suez	15	44·54	
(3)	3 „ Rodriguez	15	43·31	
	2 „ Cape	15	43·61	Stone & Finlay
	The mean is	15	43·75	

and the Cape observation differs 0″·14 from the mean.

I have given these comparisons and called attention to the close agreement between the Cape results and the mean results for all the stations because, when my observations were first sent to England, I was told that I had allowed my theoretical views to influence my observations. The comparisons here given afford my answer to the charges thus brought against me, and are a proof of the correctness of my theoretical views. An inspection of the results for the phases (1), (2), and (3) will show that no mistake need be made in referring an observation at least to its proper class. But if an observation of phase (1) at one station be combined with an observation of phase (3) at another station, the result must be an erroneous value of the solar parallax to the amount of about 3″ divided by the sum of the parallax factors for the stations under consideration; or, if an observation of what I call real contact, or phase (2), be combined with an apparent contact, or phase (3), the result must be an erroneous value of the solar parallax to about the amount of 2″ divided by the sum of the factors. But if observations of phase (2) be alone combined, the discordance should be confined within comparatively speaking small limits. The method is a strong one, but requires, of course like all other methods, to be correctly applied. The views here expressed are those upon which I acted in my rediscussion of the transit of 1769, and without their substantial adoption no discussion of the observations made in 1874 is possible.

If we attempt to obtain a value of the solar parallax from the few observations of the phases δ and η collected in the Admiralty Report with the addition of only the Cape observations, it must of course be expected that the results should run wild. The probable errors of such results must be considerable, and the theory of probable errors itself is hardly applicable in a discussion of such small groups of observations. I shall, however, exhibit the results partly as a matter of curiosity and partly to show how, by mutual elimination of error, even a few observations rapidly approximate to the true result. The five observations of phase δ lead to the value

$$8''\cdot69 \pm 0\cdot322\,\epsilon, \quad \text{or} \quad 8''\cdot69 \pm 0''\cdot161,$$

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and the seven apparent contacts to the value

$$9''\cdot31 \pm 0\cdot331\ \epsilon, \quad \text{or} \quad 9''\cdot31 \pm 0''\cdot166.$$

If we combine these results, we have

$$\pi = 8''\cdot996 \pm 0''\cdot116,$$

a result which, by its probable error, touches the value 8″·88.

If we combine the Cape observations of phase ζ with the other observations of the same phase collected in the Parliamentary Report, and use, as in my paper of January, the mean of all the contacts observed at Kerguelen for the phase ζ, we have the following equations:

No. Obs.		Residuals. (Obsd.—Compd.)
4	$x + 0\cdot202\, y = 0\cdot455$	$+0\cdot035$
4	$x + 0\cdot191\, y = 0\cdot368$	$-0\cdot053$
5	$x + 0\cdot027\, y = 0\cdot426$	$-0\cdot017$
2	$x - 0\cdot057\, y = 0\cdot793$	$+0\cdot340$
3	$x - 0\cdot100\, y = 0\cdot283$	$-0\cdot176$

These equations lead to

$$18\, x + 1\cdot293\ \ y = 0\cdot7857$$
$$1\cdot293\, x + 0\cdot3428\, y = 0\cdot53096,$$

from which

$$x = 0''\cdot446, \qquad y = -0\cdot13,$$

or, since $\pi = 8\cdot95\left(1 + \frac{y}{100}\right)$,

we have

$$\pi = 8''\cdot938 \pm 0\cdot175 \times \epsilon,$$

a result which is, I believe, within its probable error of about 0″·053.

The value which I obtained from the Ingress observations was 8″·860±0·136 ε.

The result of the introduction of the Cape observations is therefore slightly to diminish the large value derived from the Egress observations when the Kerguelen observations, which were picked up through cloud, were alone available for accelerated Egress. Of course if the Kerguelen observations are not good (I mean good in the sense of being strictly comparable with the observations made at the other stations), then the observations of Egress made in Australia may modify this result; but the value of the solar parallax must, I believe, lie between 8″·84 and 8″·92. The value of the solar parallax at present entitled to the greatest weight appears to me to be

$$8''\cdot88 \pm 0''\cdot02.$$

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Addendum.

I received yesterday morning copies of the Port Elizabeth observations, the originals of which were forwarded to England in April 1875. There are three times of contact given:

	h	m	s
(i)	7	25	13
(ii)	7	25	23
(iii)	7	25	38·5

[The correction to reduce these to Cape Mean Time is about $-18^{m}\ 6^{s}$.]

"When observation (i) was recorded the contact seemed so nearly perfect that only a thin dark line along the edge of the Sun proved the contact hardly consummated.

"When observation (ii) was recorded the appearance of the limb was as though blended in one sharp defining line: hence set down as the moment of perfect contact.

"When observation (iii) was recorded the contact at that moment may be described simply as fully perceptible or certain but with no observable loss of its circular form in any appreciable degree."

The above was the description of contact (i) in the original paper. If that description had been correct these observations would appear to have been observations of apparent contacts, but then a comparison with the Cape observations appeared to show that they must be a minute wrong. I wrote therefore in 1875, asking how long before (i) the dark line was first seen, and whether there was any possibility of a mistake having been made in the minutes. The answer was that the *dark* line was a mistake; the line should have been described as a *bright* line, and that the minutes were thought to be correct. The change of the word *dark* to *bright* of course changed the interpretation to be put on these observations, which must be attempts at real contact, and in this case the minutes are correct. If we take the mean times given for the contacts (ii) and (iii), as we have done at Rodriguez and Kerguelen, the equation of condition for Port Elizabeth is

$$15'\ 41''\cdot64 + 0\cdot564\ \delta\pi = C,$$

and this, combined with those for Thebes and Mokattam and Suez, shows that the value $8''\cdot93$ satisfies the Port Elizabeth observation. I can only account for the value $8''\cdot385$ assigned to the Port Elizabeth observation in the *Observatory* magazine by supposing that the minutes have been erroneously altered. The only ground for supposing the minutes wrong was the idea at first that the observations were apparent contacts, and this idea is disproved by the bright line seen to separate the limbs only 15^{s} before the contact. This will be clearly apparent on comparison of the phases ϵ, ζ, and η given at the different stations.

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